

April 28, 1954

Dr. Neal Groman
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Dear Dr. Groman:

Thank you very much for the manuscript sent under cover of your note of the 7th. It was indeed useful for the discussions at the Oak Ridge meeting, and will be even more so for the formal paper I am now writing as a general review of "genetic recombination in bacteria".

Have you sent, or do you plan to send, this paper to the Journal of Bacteriology? I am interested to know how best to cite it (though personal communication, or to be published, should do) but also as I would like to see it there on behalf of the Journal.

This is a very well written ms., and I could find little to criticize either in form or in substance. My most pressing suggestion on the form is that you delete the section from pp. 7-9 as indicated on the attached review sheet.

As to concept, I hope I will also have clarified my notions of "transduction" as the term applies to the present case vs. Salmonella. To my mind it is far less important that one discuss whether this is a transduction, and better to emphasize the descriptive conclusion that the role of the phage here is quite different. But as I have tried (not altogether successfully) to keep clear, genetic transduction is defined, without reference to the role of phage, as any process of transmission of genetic fragments from one cell to another, as distinct from fertilization, where an intact genome is transmitted to a zygote. Thus the Salmonella case, where a phage acts as the vector of the fragment, and the pneumococcus transformation, wherein no vector seems to be needed other than the hand of the chemist, are both sub-categories of transduction. [The term was developed before it was as clear as now seems that the pn. t. was in fact a transduction in this sense]. What to call conversion depends on how one defines "genetic fragment"—your option. "Transformation" per se means only "change" and has, for example, been applied equally to the mutations from S to R as to the more interesting R to S in pneumococcus,

Yours sincerely,

Joshua Lederberg

Groman— Evidence for the active role of bacteriophage in the conversion of non-toxigenic *Corynebacterium diphtheria* to toxin production.

P L

1 8 "intimately related to" is needlessly vague; I infer you mean "an immediate consequence of".

4 11-14. This is the same as above. The quantity of phage released by young cultures of lysogenic *Salmonella* is often (but not always) too small to effect a detectable number of transductions; you have a more sensitive system. I would delete this.

7 10 I think this is overdoing the argument, and likely to do more harm
to 3 than good, and would therefore leave it all out. Your conclusion is certain on the following brief argument: In *Salmonella*, the vectorial role of phage is shown by the separability of infective and transductive functions, i.e., not every particle accomplishes any particular transduction. In diphtheria, your well designed experiment failed to separate these activities despite several single-plaque isolations, so that one can conclude that the phage per se invariably transforms the recipient. Since transduction is defined (without reference to phage!!) as a transmission of a hereditary fragment from one cell to another, the question is ~~whether~~ not so much whether conversion is a transduction (which depends now on whether you choose to regard the phage itself as a hereditary fragment) but the role of the phage in the two systems.

In *Salmonella*, the phage is a passive vector; in diphtheria, at the extreme, (pro-) the phage would have to be regarded as the genetic element itself.

5 3 This argument relies on the implicit assumption that the C7 is itself convertible by phage grown on C7 or C4. Otherwise, two possibly non-homologous nontoxigenic strains could still interact by transduction (Cf. restorations of motility in *Salmonella* in Stocker et al.).

5 last Is the EOP of this system known?

